why such a wave should ever become blocked at all, still less that the point at which it does become blocked should be so variable in different individuals of the same species. On the other hand, if we suppose the propagation of the ganglionic influence to be more or less dependent on the presence of a more or less integrated nerve-plexus, we encounter no difficulty; for on the general theory of evolution it is to be expected that if such fibres are present in such lowly animals they should not be constant as to position.

But there is a still stronger argument in favour of nerve-fibres, and it is this. At whatever point in a spiral strip which is being progressively elongated by section the blocking of the contractile wave takes place, such blocking is sure to take place completely and exclusively at that point. Now I cannot explain this invariable fact in any other way than by supposing that at that point the section has encountered a line of functionally differentiated tissue—has severed an incipient nerve.

Such, some of you may remember, was the state of the evidence when I last addressed you upon this subject. On the whole I provisionally adopted the view that all parts of the rudimentary muscular sheet of the Medusæ are pervaded by a plexus of rudimentary nerves, or "lines of discharge;" and I explained the fact of the tissues in some cases enduring such severe forms of section without suffering loss of their physiological continuity, by supposing that all the rudimentary nerve-fibres composing the plexus to be capable, in an extraordinarily high degree, of vicarious action. If you were to represent the hypothetical nervous plexus by a sheet of muslin, it is clear that however much you were to cut the disc of muslin with such radial or spiral sections as are represented in the diagrams, you could always trace the threads of the muslin with a needle round and round the disc without once interrupting the continuity of your tracing; for on coming to the end of a divided thread you could always double back on it and choose another thread which might be running in the required direction. And this is what I last year stated to be my opinion as to what took place in the fibres of the hypothetical nervous plexus;—whenever a stimulus wave arrives at a cut, I imagined it to double back and pass into the neighbouring lines of discharge, which I thus supposed to act vicariously for the divided line.

Such, then, when last I addressed you, was the standing of this question as to the character of these highly remarkable contractile waves. On the whole I decided in favour of a rudimentatary nervous plexus, notwithstanding the improbability that such a plexus should be capable of vicarious action in all its parts to so almost unlimited a degree.[1] I am glad to say that this decision has now been further justified by some additional observations which are of the first importance. For since my last lecture I have noticed the fact that reflex action takes place between the marginal ganglia of the Medusæ and all the contractile tissues of the animal. Thus, for instance, if you seize the polypite with a pair of forceps, the marginal ganglia almost immediately set the swimming-bell in violent motion—thereby showing that the stimulus must have coursed up the polypite to its point of insertion in the bell, and then down the sides of the bell to the ganglia, so causing them to discharge by reflex action. Again, suppose that seven of the eight ganglia have been removed from the margin of *Aurelia*, and that any part of the contractile disc is stimulated too gently to start a contractile wave from the point immediately stimulated, a contractile wave will nevertheless shortly afterwards start from the ganglion—thus showing that a *stimulus* wave must have passed through the contractile sheet to the ganglion, and so caused the ganglion to discharge. Indeed in many cases the passage of this stimulus wave admits of being actually *seen*. For it is a peculiarity of the numberless tentacles which fringe the margin of this Medusa, that they are more excitable than is the contractile tissue of the bell. Consequently a stimulus may be applied to the contractile tissue of the bell which is not strong enough to start a contractile wave in the bell-tissue itself, and is yet strong enough to start a contractile wave in the tentacles—one tentacle after another contracting in rapid succession until the wave of stimulation has passed all the way round the disc. The latter, of course, remains quite passive until the tentacular wave, or wave of stimulation, reaches one of the ganglia (or the single remaining ganglion, if the disc has been prepared by removing seven of the ganglia), when, after an interval of half a second for the period of latency, the ganglion is sure to discharge, and so to cause a general wave of contraction.

Now these facts prove, in a singularly beautiful manner—for this optical expression of the passage of a wave of stimulation is a sight as beautiful as it is unique—these facts, I say, conclusively prove that the whole contractile sheet of the bell presents not merely the protoplasmic qualities of excitability and contractility, but also the essentially nervous quality of conducting stimuli to a distance irrespective of the passage of a contractile wave. So I conclude there can be no longer any question that we have here to deal with a tissue already so far differentiated from primitive protoplasm, that the distinguishing function of nerve has become fully established.

[1] This antecedent improbability is not so overwhelming as it is at first sight apt to appear; for we must remember that in a peripheral nervous plexus as we meet with it in the higher animals—*i.e.*, in the fully evolved form of such a structure—each of the constituent nerve-fibres is provided with an insulating coat for the very purpose of preventing vicarious action among these fibres and the consequent confusion among the reflex mechanisms which such vicarious action would manifestly occasion. But because insulation of peripheral nerve-fibres is thus an obvious necessity in the case of a fully evolved nervous plexus, it by no means follows that any high degree of insulation should be required in the case of an incipient nervous plexus. On the contrary, any hypothesis as to the manner in which nerve-fibres first begin to be differentiated from protoplasm must suppose that the conductile function of the incipient nervous tracts precedes any structure, such as that of nerve-coats, whereby this function is strictly confined to particular tracts. The antecedent probability being thus in favour of the view that insulating structures are a product of later evolution than are the essential nervous structures which they insulate, it would clearly be very hazardous to draw any analogy between an incipient nervous plexus such as I suppose to be present in the Medusæ, and a fully-evolved peripheral plexus of any of the higher animals. A less hazardous analogy would be furnished by the fibres which occur in the *central* nervous system of the higher animals; for here it may be said, both *à priori* from Mr Spencer's theory and *à posteriori* from histological indications, that the nerve-fibres occur in various degrees of differentiation. And that vicarious action is possible to some considerable extent through a bridge of the grey matter of the cord, has been shown by the double hemi-section experiments of Brown-Séquard. Moreover, the admirable experiments of Goltz would seem to indicate that vicarious action is also possible to a large extent among the ultimate elements of the brain. I may add that recent research has tended to suggest a novel interpretation of the way in which certain poisons, such as strychnia, act upon the cord; for whereas it has hitherto been supposed that the abnormal reflex excitability which these poisons engender is due to their exerting a stimulating influence on the cord, the researches in question have fairly well proved that the very reverse is true, viz., that the action of these poisons is to *depress* the vitality of the cord. For a number of facts go to prove that the abnormal reflex excitability is due to the impairment of some function which has been provisionally termed "resistance of the cord," a function which in health prevents the undue spread of a stimulus through the substance of the cord, and the impairment of which by the poison consequently admits of a stimulus spreading to an undue extent, so giving rise to the abnormal reflex excitability in question. As bearing on this subject, I may observe that while the action of strychnia on the Medusæ is the same as it is on the higher animals, viz. that of causing paroxysmal convulsions, it certainly seems to exercise a *depressing* influence on the tissues; for an extremely weak sea-water solution has the effect of blocking contractile waves in any part of a spiral strip that is submitted to its influence.—G. J. R.

THE NORWEGIAN ATLANTIC EXPLORING EXPEDITION

Tromsö, July 13, 1877

THE expedition met at the beginning with several unfavourable circumstances. In the last week of May Capt. Wille went out to Husö with the *Vöringen*, in order to determine the magnetical constants of the ship. After his arrival a flaw was discovered in the shaft, so that he went back to Bergen, where there was fortunately a new shaft lying ready. A few days later the ship was again at Husö, and was swung, not without some difficulty owing to rough weather. The *Absolute* magnetical observa-

tions on shore were secured by Capt. Wille. The necessary observations for compass error having been obtained, the *Vöringen* returned to Bergen, where the scientific staff was assembled. There was, however, something still wanting before we could put to sea. The accumulators used last year had got brittle, and new ones had been ordered from London in March, but they had not arrived in May, and in answer to a telegram Capt. Wille learnt that the order had been forgotten. The new accumulators kept us waiting in Bergen till June 11, when we sailed for Stavanger, and received them on the 13th, and we put to sea at once.

Outside the coast we took a series of temperatures, which showed the minimum, not at bottom, but at a certain depth below the surface. The same phenomenon has lately been observed in all latitudes near the coast. I attribute it to the action of the winter cold on the sea.

Our first working station was in lat. 66° 8′·5, long. 3° 0′ E., which was reached on the morning of June 16. The depth here was 805 fathoms, the temperature at bottom, 29°·7. We now worked in even sections, running west-north-west and east-south-east perpendicular to the coast. The third section from lat. 67° 53′, long. 5° 12′ E. to the island of Trocuew having been completed, we went northwards into the West Fiord, where a series of temperatures was taken with Negretti and Zambra's deep-sea thermometer. Last year we could not use this instrument at sea because the slightest upward movement of the ship caused the thermometer to turn over before it had had sufficient time to accommodate itself to the temperature of the sea. This year it was fitted with a new turning apparatus devised by Capt. Wille, which proved satisfactory. In the outer part of the West Fiord the temperature on the surface was 45°·7; it decreased to 38°·8 in sixty fathoms; and from that point it rose to 41°·0 in 140 fathoms, ten fathoms above the bottom. The Casella-Miller thermometer of course registered from this depth the minimum 38°·8. The phenomenon here noticed is universal all along our coasts in the summer months; I discovered it for the first time in the West Fiord two years ago. The explanation seems to be this: In winter the air is generally cooler than the sea-surface, especially at the coast; the water is chilled from above, and the cooled layers being denser, sink down, and so the winter cold descends in the water; the temperature down to a certain depth increases with the depth. When spring and summer come, the air warms up the sea surface, and the surface layers getting warmer get lighter also, and have no tendency to sink. The temperature becomes highest at the surface, and decreases to a certain depth, below which the action of the winter cold still shows itself in a temperature increasing with the depth.

After dredging and trawling in the inner part of the West Fjord, we went to Boelö, where the expedition stayed a couple of days. On the 26th we arrived at Rösh, the outermost of the Loffoden Islands; there we stayed some days, strengthening the accumulators, cleaning the ship, taking magnetical and astronomical observations, and making excursions.

We left Rösh on the 28th at noon, and commenced our work on the sections further north, sounded, dredged, and trawled outside the Loffoden Islands till the 30th, when we went into the Hadsel Fjord, and anchored at Sortland in Westeraalen. The next week was spent in working outside Westeraalen. There the greatest depth for this year was reached, 1,710 fathoms in lat. 70°, long. 6° 15′ E. The Casella-Miller thermometer registered at the bottom a temperature of 28°·4 when corrected for instrumental error and for pressure, the lowest temperature hitherto found by our expedition. A series of temperature observations showed that the temperature at all depths decreased with the depth, and that 32° lay in about 580 fathoms. The next Sunday, July 8, found us in Tromsö.

The expedition has this summer been favoured with remarkably fine and quiet weather, which has allowed us to carry out all our operations according to our proposed plan. The number of sounding stations is already 101; last year's total was only 93. Seventeen serial temperatures have been obtained, and the dredge and trawl have been out on the bank in the *Umbellularia* region (one specimen has been caught), and in the deep *biloculina* clay, at the depth of 1,700 fathoms, animal life was rather scarce. The boundary line between the water above and below 32° at the bottom, lies between lat. 65° and the Arctic circle as far west as 5° 30′ E. A little north of the Arctic circle it has a curvature towards the coast, and farther north it lies only from five to ten geographical miles off the outer side of the islands of Loffoden and Westeraalen. On this northern part the edge of the bank is very steep, and the bottom falls very rapidly towards the deep part of the Arctic Ocean. Out at sea the isothermal surface of 32° lies at very different depths in different latitudes. In the channel between Faroe and Shetland, it lies in 300 fathoms, between Iceland and Norway it sinks to 400 fathoms, and between Jem Mengen and Norway we have found it in 580 fathoms. To the westward it rises, as we found last year, east of Iceland. How it behaves further north, off Spitzbergen, we expect to find next year. Near the coast, 32° always lies at a much higher level.

The *Umbellularia* region has been found extending as far down as 880 fathoms off Westeraalen, where the specimen found came up with the weights on the dredge rope. In several places off the coast we have found, besides Norwegian rocks, specimens of chalk and flint. Of deep-sea animals, some new species have been found. On the bank off Langenes (lat. 69°) we caught plenty of the same kind of fish as are caught at the bank fisheries on the "Storeggen," off the coast of Romsdal.

The expedition is now lying at Tromsö, refitting and taking in stores for further work. We intend first to work only two more sections north of Tromsö, and then call here to make all ready for the voyage to Jem Mengen. From that island the course will be westward till we reach ice-cold water, then southwards to a point midway between Jem Mengen and Iceland, and thence to Bodö, whence the expedition will return to Bergen.

Among the novelties used in our work this year I must mention a piezometer, kindly sent me by Mr. Buchanan, chemist of the *Challenger*. This instrument has registered the depth very well. A new atmometer of my own construction has been constantly in use, giving good results. Two such instruments have, under favourable circumstances (they cannot be used in rough weather), given almost identical results; the depth of sea-water evaporated in twenty-four hours is sometimes more than four millimetres. Meteorological observations have been made every hour when at sea. The chemist has got many samples of air from the sea-water, both at the surface and at the bottom. He has taken the specific gravity of the water and determined its amount of chlorine. He has also made several determinations of its amount of carbonic acid.

MR. FROUDE'S NEW DYNAMOMETER

MR. FROUDE, in solving the problem assigned to him by the Admiralty—of producing a dynamometer calculated to test the power delivered at the end of the screw-shaft by large-sized marine engines—has enabled us to utilise a new principle of great value among the "applications of science."

In the friction-brake dynamometer, as is well known, the power delivered to a revolving shaft is measured by the rate at which a definite weight is being virtually lifted, and the number of foot-pounds of work done per minute is the circumference of the drum at the effective radius at which the weight is lifted, multiplied by the weight and by the number of revolutions per minute. Simple as the arrangement is when employed on a small scale, it